THURSDAY, OCTOBER 23, 1879

THE INTRA-MERCURIAL PLANET QUESTION

IN No. 2253-54 of the *Astronomische Nachrichten*, Dr. C. H. F. Peters, the discoverer of so many minor planets, has "Some critical remarks on so-called intra-Mercurial planet observations," including the observations reported by Prof. Watson during the totality of the eclipse of July 29, 1878. Replies to these remarks have since appeared from Prof. Watson and also from Mr. Lewis Swift, of Rochester, New York, who, it will be remembered, also considered he had seen an object which could be no known star or planet. Prof. Peters enters upon other cases where intra-Mercurial bodies have been suspected, but we shall confine ourselves mainly here to his criticism of Prof. Watson's observations during the last eclipse. His object is to adduce evidence disproving Watson's conclusion that he had seen one, probably two unknown planets, and he grounds his argument chiefly upon the small size of the circles to which Watson trusted, and the fact that nearly on the parallel of his two objects a and b, and at an almost equal distance, a small one, in right ascension, were the stars θ and ζ Cancri. He states that the circles of wood with paper scales pasted on them, were only 5 and $4\frac{3}{4}$ inches respectively in diameter, and as Prof. Watson estimated the probable error of a position given by them at only five minutes, the space would measure on the circles only $\frac{1}{215}$ of an inch, and further he states that the wires which served as pointers "were so elastic as easily to give way several degrees under the touch by a pencil." So far, therefore, from accepting Watson's estimate of the precision of his readings, Prof. Peters thinks he does him no injustice in supposing that they were made "at the utmost to $\frac{1}{70}$ inch, corresponding to twenty minutes of arc upon his circles;" and in this case, the differences of a from θ Cancri, and of b from ζ Cancri, or + 2m. 55s. and + 3m. 23s. respectively, he believes may be explained by the errors in the markings or readings. It is also urged that the markings for the sun were made under circumstances less hurried than those for the suspected planets. Watson estimated the objects at the time of 4 and $4\frac{1}{2}$ magnitude, and, remarking that absolute magnitude must be quite uncertain under such conditions, Peters points out that the difference of brightness corresponds pretty nearly with that given by Argelander, Heis, and others between ζ and θ Cancri; and he adds, "it is, therefore, quite apparent to every unbiassed mind that Watson observed θ and ζ Cancri, nothing else." It should be added that Prof. Peters attempts to explain the ruddiness of the object near θ,—"If the sand ledge, under the lee of which the telescope was standing had nothing to do with it;" the observation, perhaps, teaches that the corona possesses the property of less absorbing the red rays, and may, therefore, be of some value. It seems also, in his view, that the corona gives a disk to the stars, or calms down the radiations to a kind of spurious disk, as a slight fog does; and as he does not admit that the power employed would show a real disk, we are to assume this was the case during Prof. Watson's observations. With regard to Mr. Swift's observations, it is represented that his successive publications offer so singular a gradation in the statements as to unfit them to be the subject of a scientific discussion.

As we have stated, Prof. Watson has replied to the criticisms which the Clinton astronomer has published to "make evident beyond cavil how erroneous the conclusion too rashly rushed at by the observers." He protests against mis-statement of the facts connected with his observations, remarking that it appears to him "the grossest of unfairness to attempt to discredit an observation made by an experienced observer by deliberately misrepresenting the circumstances of the observations." So far from the pointers of his circles bending under the touch by a pencil, they were made of unannealed brass wire one-eighth of an inch in thickness, not filed to a point, but to a *knife-edge*, placed vertical to the plane of the circle; they were quite rigid, and could not be disturbed in the least by the pencil when marking. The probable errors attributed to his readings Watson declares to be absurd, and says that any one interested may, by a few trials ascertain that by the method he adopted it is possible to measure without a greater probable error than 2′; the limit of 5′ which he gave was an outside one. Peters had urged that a practised observer would have compared the object a directly with θ Cancri, as the two would have been in the field together with the telescope employed, to which Prof. Watson replies, and with justice, that besides the want of time for such direct comparison, the method he was applying was different. If he had known that a new star would present itself near θ Cancri, he could have prepared himself for direct comparison; under the circumstances his plan of securing rapid indication of the position of any object that might be visible seems to have been as effective a one as could have been devised, and, as Dr. Draper termed it when it was explained to him the night before the eclipse, "a good dodge." Further, Watson observes that the assertion that his circles "were of wood, with paper scales pasted on to them, and wires serving as pointers," shows conclusively that Peters either did not yet understand his method, or that he was "purposely mis-stating the circumstances of his observations."

Finally, he makes what he terms these emphatic declarations:—"1. I observed, during the total eclipse of July 29, 1878, a new star between θ Cancri and the sun, and south of the sun, whose position and magnitude were as already published by me. 2. I observed another star, which I believe to be a new star, whose magnitude and position were as already published by me." Whether or not these objects were intra-Mercurial planets he does not positively assert, but he had the right to express the hones belief that they were. Watson adds that he "hopes ere long to give good reason for the faith that is in him," by which we understand him to imply that he has the intention to enter further upon the subject.

We will venture to say that the general feeling amongst astronomers when first reading Prof. Watson's announcement of his observations during the totality of the eclipse of 1878, of one, if not two, unknown objects, would be that a man of such known ability and experience as an observer, and so good a practical astronomer, as shown, amongst other proofs, by his able treatise on practical astronomy, would not risk his whole scientific reputation by putting forth such a statement to the world, unless he

was firmly convinced of its truth and felt able to substantiate it; otherwise the fact that there were two known stars, on the parallel or nearly so, and less than one degree west of the objects supposed to be new, would probably have been felt to be an almost fatal objection to the reality of the discovery. It must be remembered that Watson asserts he did see θ Cancri as well as the neighbouring object a; Peters objects—"not at the same time though"—an objection which Watson does not notice in his reply, but which will be easily removed by him; it might perhaps be rather gathered, that if the objects were not in the field together, he satisfied himself of their distinctness.

Throughout Prof. Peters's criticisms, not only as regards the American astronomer's observations during the eclipse, but other reported observations of unknown bodies in transit over the sun's disk, there is evinced a certain *animus*, which might have been as well avoided, and there is a flippancy in his reference to Leverrier's labours on the theory of Mercury, which he hopes "will be investigated anew by a hand very favourably known in this field of research, and we may expect then to have the intra-Mercurial spectre put to rest definitively." Most astronomical readers will feel more respect for the opinion of our great physical astronomer, Prof. J. C. Adams, who, on presenting the gold medal of the Royal Astronomical Society to Leverrier in 1876, thus expresses himself as to the existence of intra-Mercurial matter, as indicated by the French astronomer's researches—"The theory of the planet has been established with so much care, and the transits of the planet across the sun's disk furnish such accurate observations, as to leave no doubt of the reality of the phenomenon in question; and the only way of accounting for it appears to be to suppose, with M. Leverrier, the existence of several minute planets, or of a certain quantity of diffused matter circulating about the sun within the orbit of Mercury."

It has been mentioned that Mr. Swift has also addressed a communication to the *Astronomische Nachrichten*, in consequence of Peters's criticism of the observations made during the eclipse. Mr. Swift notified, soon after the occurrence of that phenomenon that he had seen two reddish objects with sensible disks about 3° distant from the sun; their mutual distance he first stated to be twelve minutes of arc, subsequently correcting this estimate to seven or eight minutes, as it is given in a letter which he addressed to NATURE, vol. xviii. p. 539, but in the same letter, referring the position of one object to that of the other, which he believed to be θ Cancri, by means of the place of the star given by the Astronomer-Royal in a communication to this journal, he assigned a position which, as we pointed out (vol. xviii. p. 569), would locate the supposed planet at a distance of thirty minutes from the star, instead of seven or eight minutes. He now writes that the difference of declination (? right ascension) shown by his own and Watson's observations, had been "a source of solicitude," as he could see no way to harmonise them "till NATURE pointed out the error of reducing the eight minutes of arc to time, saying it was but 32s. instead of 2m. This changed the whole complexion of the matter. The scales immediately fell from my eyes, and for the first time I was able to see my way clearly through the difficulty with which it had so long been enshrouded." We should have been glad if we could explain in what manner the sudden illumination consequent upon our remarks reconciles the distances in question, and so clear the way for accepting Mr. Swift's observation as confirmatory of that of Prof. Watson. He tells us that he has been an observer of the heavens for twenty-two years, and we know that he has been the first discoverer of several comets, and have no intention to depreciate his claim to credence on any astronomical question, but it has naturally happened that the different statements and the hesitation felt as to the distance of the objects he observed has detracted from the importance which would otherwise have attached to his experiences during the eclipse.

AUSTRALASIA

Australasia. Edited and Extended by A. R. Wallace. With Ethnological Appendix by A. H. Keane, M.A. (London: Stanford, 1879.)

THIS stout octavo volume is one of the series entitled "Stanford's Compendium of Geography and Travel," based on Hellwald's "Die Erde und ihre Völker." Mr. Wallace tells us, however, in the preface that he has been able to utilise comparatively little of the translation of Hellwald's work, so that it forms little more than one-tenth part of the present volume.

The term Australasia is taken in a very wide sense to include the entire East Indies and the Philippines, New Guinea and Australia, and all the islands of the Pacific, even to Easter Island. The region extends thus around much more than one-third the circumference of the globe.

The book commences with a short general account of the main geographical and biological features of the area and then treats of its various subdivisions separately. The author divides his Australasia into six principal regions, viz., Australia, Malaysia, Melanesia, Polynesia, Micronesia, and New Zealand. He commences with a very interesting summary of the principal physical features and climatic conditions of Australia.

Australia with Tasmania is only a little less in area than Europe. Yet its highest mountain, Mount Kosciusko, is only 7,308 feet in height. Its greatest river, the Murray, has a basin the area of which is about equal to that of the Dnieper. The hottest climate in the world probably occurs in the desert interior of Australia. Capt. Sturt hung a thermometer on a tree shaded both from sun and wind. It was graduated to 127° F., yet so great was the heat of the air that the mercury rose till it burst the tube, and the temperature must thus have been at least 128° F., apparently the highest ever recorded in any part of the world. For three months Capt. Sturt found the mean temperature to be over 101° F. in the shade. Nevertheless in the southern mountains and tablelands three feet of snow sometimes falls in a day; in 1876 a man was lost in the snow on the borders of New South Wales. Snowstorms have been known to last three weeks, the snow lying from 4 to 15 feet in depth and burying the cattle. Forty miles of the railway from Sydney to Bathurst have been seen covered continuously with snow. Australia is the land of drought and flood. The annual rainfall at Sydney has varied from 22 to 82 inches. Lake George, near Goulburn, was, in 1824, 20 miles long and 8 miles broad.